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V. *On the Relation which subsists between the Nervous and Muscular Systems in the more perfect Animals, and the Nature of the Influence by which it is maintained.* By A. P. W. PHILIP, M.D. F.R.S. L. & E. &c.

Read February 7, 1833.

IN the last paper which I had the honour to present to the Society, and which appeared in the Philosophical Transactions for 1831, I endeavoured, by comparing the various facts relating to the circulation of the blood, and by such additional experiments as seemed to be required, to free the subject from the confusion in which it had been involved by the various and contradictory experiments and statements of writers, and to ascertain the source and nature of the powers on which the motion of the blood depends.

In the present paper I propose to consider in the same way, another subject of equal importance, intimately connected with the preceding, and which has, by the same means, been involved in equal, and, from its more complicated nature, apparently greater perplexity; namely, the relation which subsists between the nervous and muscular systems, and consequently, between the nervous system and organs of circulation; for I think it will be admitted, from the statement of facts made in the paper just referred to, that the power of the vessels, like that of the heart, is a muscular power, and that on the combined power of the heart and vessels, the motion of the blood, in the ordinary states of the circulation, wholly depends. Having considered this part of the subject, I shall endeavour to point out how far we can proceed in ascertaining the nature of the nervous influence, the means by which the relation between the nervous and muscular systems is maintained.

I need hardly observe that in experimenting on a subject so complicated as the living animal body, much practice as well as caution is necessary in order to guard against erroneous conclusions, and thus obtain for physiology the certainty which can alone entitle it to be regarded as a science. There is none of its branches which has not suffered from the inaccuracy of experi-

mentalists, and, from the complicated nature of the subject, none which has suffered more in this way than that to which I am about to call the attention of the Society.

Throughout the long inquiry in which I have been engaged, I have always been more anxious to secure any ground I had gained by a careful repetition of the experiments, than to proceed to further attempts; and I may be allowed to state, that although my experiments have been repeated by many of the first physiologists, both of this country and the continent, they have not in any instance been found inaccurate. The rules I have always followed, and which, if I may presume so far, I would recommend to all engaged in such inquiries, are to make the experiments in the presence of competent witnesses, and to repeat them as long as any doubt remains on the mind of any individual present. Were these rules strictly observed, physiology would cease to be perplexed by the inaccuracies in which the inexperienced and precipitate are constantly involving it.

But this has not been the only cause of perplexity in the subject I am entering on. The evil, both here and in other branches of physiology, has, in a great degree, arisen from writers either having been too apt to enter upon their inquiries and detail their experiments without having made themselves acquainted with the state of our knowledge at the time, and without having sufficiently weighed what had been done by others, even as far as they were acquainted with it; or, when little had previously been done, having drawn their inferences from too partial a view of facts. Thus, much confusion and contradiction have often been introduced even where the immediate results of their experiments were accurately observed and recorded, and they have been prevented from so directing their labours as to conduce to a gradual and steady accumulation of knowledge.

As the early physiologists present to us the results of their own imaginings, without even pretending to be possessed of any facts which directly support them; the later and more rational inquirers have, from the causes just enumerated, been often betrayed into conclusions equally erroneous: and even in making important additions to our knowledge, they have frequently done what they could to render them useless and sometimes injurious to the cause of science, which inferences, from a partial view of facts, often are, to a greater

degree than those which are evidently suppositions, because they assume the semblance of legitimate deductions.

Thus it was that M. LE GALLOIS taught that the power of the heart is independent of the brain, but derived from the spinal marrow, and that the powers of circulation in every part of the body depend on the corresponding part of that organ; inferences apparently supported by the facts he adduces, but wholly inconsistent with others which escaped him: and yet, at first view, so much the necessary results of his ingenious experiments, all of which were correct, that the Royal Academy of Sciences at Paris, after they had been repeated in the presence of the celebrated HUMBOLDT and other eminent academicians appointed by the Academy to witness their repetition, adopted all his conclusions, and were for some time followed by the learned of other countries. Many similar instances might be adduced*.

IN order that I may distinctly lay before the Society the nature of the first question to be considered in the following paper, it is necessary to observe that it appears from experiments detailed in papers which the Society did me the honour to publish in the Philosophical Transactions for 1815, and since republished in my Inquiry into the Laws of the Vital Functions, that the power both of the muscles of voluntary and involuntary motion is independent of the nervous system, yet in both equally capable of being influenced by it, the nervous influence being the constant stimulant in the functions of the former class of muscles, and an occasional stimulant in those of the latter, which in all their usual functions are excited by stimulants peculiar to themselves; but that these classes of muscles are influenced by it in very different ways, each of the muscles of voluntary motion being under the influence of no part of the brain and spinal marrow, but the particular part from which its nerves arise; while each of the muscles of involuntary motion is under that of every part of

* It was thus that HALLER inferred, from finding that the heart cannot, like a muscle of voluntary motion, be excited through its nerves, that this organ cannot be directly influenced by the brain and spinal marrow, an error which has more extensively influenced the practice of medicine than any other into which physiologists have led us. The fact on which this inference is founded depends not on the heart being placed beyond the direct influence of the brain and spinal marrow, but on the nature of the ganglionic nerves.

these organs, from the upper surface of the brain and cerebellum to the lowest portion of the spinal marrow.

In subsequent papers, published in the *Philosophical Transactions*, and in the experimental inquiry just referred to, I endeavoured to trace the final cause of this arrangement; and found that the functions, with the exception of the circulation, to which the muscles of involuntary motion administer, namely, secretion and the other assimilating processes, require for their due performance the united influence of every part of the brain and spinal marrow; while the muscles of voluntary motion are concerned in no function but that of giving motion to the different parts of the body to which they are attached, and consequently have no direct influence on the functions of life.

From the whole of the experiments on which is founded the view I have thus been led to take of the functions of the nervous system, and its relation to the different classes of muscles, it would appear that the brain and spinal marrow are the only active parts of that system; the nerves, whether cerebral or ganglionic with their ganglions and plexuses, being only the means of conveying and combining the influence of the various parts of these organs.

But as these inferences are less direct than those arrived at in the foregoing publications, and as very different opinions have been maintained both by preceding and subsequent physiologists, it appears necessary to review this part of the subject with more care, particularly as our view of many of the phenomena, both of health and disease, must be essentially modified by it. This therefore is the first question I propose to consider.

The various parts concerned in the functions of the living animal may be divided into active and passive, those in which the power resides, and those which only obey that power, but which are equally essential to the function. Thus, the belly of a muscle is the active; the tendon, the passive part of the organ. In the production and application of the bile, synovia and other fluids prepared for the purposes of the animal economy, the gland is the active, the ducts, as far as the peculiar office of the organ is concerned, the passive part of that organ; and I know of no way of ascertaining which is the active and which the passive part of any organ, but by observing which obeys the other in the function of the part.

Thus it has been pretty generally admitted with respect to what may be

called the cerebral part of the nervous system, that is, certain parts of the brain and spinal marrow, with the nerves immediately proceeding from them, that the former are the active, the latter the passive parts of these organs; because we find that the power of the nerves is always proportioned to the excitement of the brain and spinal marrow, as that of the tendon is proportioned to the excitement of the belly of the muscle. Even this position however has by some been controverted, and it has been maintained that the nerves themselves in part supply the influence on which their functions depend.

But in whatever manner this part of the question, which it will be necessary to consider more particularly, may be disposed of, there is certainly, from the more complicated structure of the parts, better reason for regarding the ganglionic nerves with their ganglions and plexuses, as active in the formation of nervous influence, than the simple nerve which connects the cerebral mass with the muscle which it excites; and it has consequently been the opinion of many physiologists that the ganglionic system is concerned in the production of this influence, and some have gone so far as to regard it as independent of the brain and spinal marrow, and therefore the only source of the power of its own nerves.

As soon as it was found that the organs supplied with ganglionic nerves obey every part of the brain and spinal marrow, it was necessary to abandon the latter opinion, and we could see a reason for the complicated structure of the ganglionic system, independently of its supplying any part of the nervous influence. As each of the vital organs is sensible to the action of every part of the brain and spinal marrow, some apparatus capable of combining the influence of all these parts is evidently necessary, and none apparently could be better fitted for the purpose than that system, which, both by its ganglions and plexuses, and the frequent anastomoses, if I may use that expression, of its nerves, seems even at first view intended to combine the power of the various parts from which it receives nerves; and when those proceeding from the ganglions and plexuses are found by direct experiment to convey the influence of all those parts, the inference appears almost unavoidable.

We here have a proof that the organs supplied by ganglionic nerves obey the influence of all parts of the brain and spinal marrow, and consequently that the ganglionic nerves combine and convey the influence of all those parts;

and it is contrary to what we observe of the simplicity of the operations of nature that there should be another source of that influence.

In another fact we find an additional objection to such a supposition, for it appears from many experiments related in the papers and inquiry above referred to, that exactly in proportion as we increase or impair the power of the brain and spinal marrow, the functions of the ganglionic nerves are increased or impaired; still pointing out the brain and spinal marrow as the active, and the nerves with their ganglions and plexuses as the passive parts of the system; and these observations come with the more weight because those who have maintained that the ganglions supply nervous influence, have not even pretended to support their opinion by any facts directly bearing on the point.

If however it also appears from direct experiment that the ganglions and plexuses are capable of influencing the power of the ganglionic nerves, independently of any change induced on the brain and spinal marrow, however improbable the fact may at first sight appear, we must admit that there is in the former organs an additional source of the power possessed by those nerves.

It has been found that the action of the heart is immediately influenced by agents, whether stimulants or sedatives affecting any considerable part either of the brain or spinal marrow*. Can its action, in like manner, be affected by agents making their impression on the ganglions and plexuses? For the purpose of determining this point, the following experiment was made, in which Mr. CUTLER, and Mr. FIELD the well-known veterinary surgeon, were so good as to assist me, Mr. FIELD performing the operative part.

The heart continues to obey the effect of agents acting through the nervous system for a certain time, after what we call death, that is, the removal of the sensorial powers†; and this time is much prolonged if the circulation be maintained by inflating the lungs at proper intervals; for in all modes of death, except where the nervous system is so powerfully and suddenly impressed as at once to destroy all the functions, the nervous as well as the muscular survive the removal of the sensorial powers‡; and the newly dead is on several

* Papers in the Philosophical Transactions for 1815, and Experimental Inquiry into the Laws of the Vital Functions.

† Philosophical Transactions for 1829, and Experimental Inquiry.

‡ Ibid.

accounts a better subject than the living animal, for such experiments as the following, although the result is still more satisfactory if the animal can be so prepared as to destroy the sensibility as far as the experiment is concerned, without so completely destroying it as to interrupt respiration*.

Mr. FIELD partially divided the spinal marrow near the head in an ass in such a manner as to destroy the sensibility, as far as the experiment was concerned, but not to interrupt the respiration, thus bringing the animal into the best possible state for the experiment. It lay as still and suffered as little during it, as an animal quite dead in the usual sense of the word, while the circulation was more perfect than it could be under any artificial inflation of the lungs. In another respect, the state of the animal was particularly favourable, for Mr. FIELD succeeded in exposing the semilunar ganglion and its plexuses with a very trifling loss of blood, not I believe four ounces. The heart was then found to pulsate sixteen times in ten seconds, as ascertained by the pulsation of the arteries in the neighbourhood of the ganglion. The ganglion and its plexuses were then irritated by the point of the scalpel, and at length cut across in various directions; but although the beats of the heart were repeatedly counted during these operations, they continued uniformly of the same frequency. Spirit of wine was then applied to the wounded ganglion and plexuses, but without the least change in the beats of the heart. A strong infusion of tobacco in water was now applied, but with the same result, the beatings of the heart being still sixteen in ten seconds; nor could any variation in the force of the beats be observed in any part of the experiment.

It appears from this experiment that we cannot influence the organs supplied by the ganglionic nerves by causes affecting the ganglions and plexuses, independently of the brain and spinal marrow; and the inferences from this and the preceding facts are unavoidable, that the former organs make only a part of the channel through which the influence of the latter is conveyed; and that the peculiar office of the ganglions and plexuses is to combine the influence of the nerves which terminate and are blended in them, and send off nerves endowed with their combined influence, in consequence of which the

* It has been shown in the publications just referred to, that in the more perfect animals, respiration is as much a function of volition as the motion of a limb, and consequently ceases when the sensibility is wholly destroyed.

parts which receive the nerves proceeding from them, become subject to every part of the brain and spinal marrow.

Such being the case with respect to the ganglions and plexuses, it is not likely that we shall find the nerves themselves, whether ganglionic or cerebral, capable of supplying any part of the influence they convey; but that nothing may be taken for granted, this also is a point which must be determined by an appeal to facts.

It is to be recollected that here, as in other cases, the *onus probandi* rests with theasserter. This would still be the case, although his position were less improbable than that, while there is an evident and acknowledged source of nervous influence, and that adequate to the production of the phenomena, another source of it should exist. Those who maintain such an opinion must adduce the proofs of it. Let us inquire to what they amount.

While the connexion of the nerves with the brain and spinal marrow exists, the nerves are capable of exciting the muscles, causing the evolution of caloric which supports animal temperature, forming the secreted fluids from the blood, and supporting the other processes of assimilation by which the structure of the various organs is maintained*; but as soon as this connexion is intercepted, all these functions begin to fail, and soon cease, nor do we possess a single fact to prove that there are any means in the nerve itself of maintaining or renewing any of them. By mechanical impulse the power which remains in a separated nerve of the cerebral class, for even this is not the case with respect to the ganglionic nerves†, may be directed to its extremities and made evident by the excitement of the muscle in which it terminates; but independently of such an impulse we have no means of exciting a nerve separated from the brain and spinal marrow, even during the short time it retains the influence it has received from those organs‡.

The very circumstance of the nerves being the means of conveying the influence of the brain and spinal marrow affords a presumption that they are not

* Philosophical Transactions for 1829, and Experimental Inquiry.

† It is true that the heart has been excited by galvanism through the medium of its nerves; but they may here act merely as conductors.

‡ In the living animal a nerve cut off from direct communication with the brain and spinal marrow but otherwise uninjured, will, as Mr. BRODIE has shown, long retain this power, as we should *à priori* have expected. It retains its healthy structure, and its communications with other nerves.

themselves the source of a similar influence. The former is evidently their peculiar function, and it is so improbable that they should perform another of so different a nature, that it would require the most unequivocal proof of such a fact to induce its belief. The power of the nerves is not only as far as we see derived from the brain and spinal marrow, and soon ceases and cannot be renewed when they are separated from these organs, but is, as I have already had occasion to observe, at all times proportioned to the degree of excitement in them; nor can an instance be adduced in which a cause of increased nervous power makes its impression on the nerves themselves. For its degree as well as existence, then, the power of the latter depends wholly on the former organs; and this observation applies as strictly to the ganglionic as to the cerebral nerves. The brain and spinal marrow therefore possess all the characteristics of the active, the nerves of the passive parts of the system.

It may appear at first sight that the phenomena of what has been termed the sympathy of nerves oppose the preceding views. On a careful review of these phenomena, however, they will be found to afford them additional support. They are all such as depend on changes in the central parts of the nervous system, and in no degree on any influence of the nerves on each other in their progress. As I have nothing to add to the statements I have already published on this subject, for the facts on which the foregoing positions are founded I beg to refer to the 106th and 107th pages of the third edition of my *Inquiry into the Laws of the Vital Functions*.

Another opinion respecting the function of the nerves has been maintained, and lately by a writer of great respectability*, which deserves to be considered, because it claims the support of experiment, and if well founded must essentially affect our opinion of the nature of the nervous influence.

Dr. HENRY appears to admit the independence of the muscular power, but thinks he has rendered it more than probable that the nervous influence, instead of being only one of many agents, is the only one capable of exciting the muscular fibre; and consequently that all others act through it, so that they are not in fact stimulants to that fibre, but to the nerves alone, through which they influence it.

* Dr. WILLIAM CHARLES HENRY'S *Critical and Experimental Inquiry into the Relations subsisting between Nerve and Muscle*, in the 110th Number of the *Edinburgh Medical and Surgical Journal*.

It is true that as mechanical impulse affecting a nerve of voluntary motion is capable, after its separation from the brain and spinal marrow, of exciting, through it, the muscle in which it terminates, and we cannot be assured that we have separated from the muscular fibre the whole of the nerves with which it is so intimately blended; if we were in possession of no other facts on the subject, we should be led to the inference, that the excitability of the muscular fibre can only be influenced through its nerves; but when instead of a mechanical, we employ a chemical agent, we find the result very different. We attempt in vain to influence a muscle through the nerve which terminates in it by the most powerful agent of this description, yet such an agent when applied to the muscular fibre itself excites it as readily as the mechanical agent, which is supposed only to affect it through the nerve.

Even if the power of the chemical agent be gradually increased until the structure of the part of the nerve to which it is applied is destroyed, not only the muscle in which it terminates, but even the other parts of the nerve itself remain wholly unaffected. The nerve has not even the power of communicating the change to its adjoining parts. This was proved by the experiments of FONTANA, and confirmed by those of Dr. HENRY related in the paper just referred to.

What reason then have we for supposing, when a chemical agent applied to the muscular fibre excites it, that it operates through the nerves which still adhere to it. Such an inference implies that nerves in their progress wholly change their nature, a supposition for which there is not only not a shadow of proof, but against which the most convincing proofs, which analogy can supply, present themselves.

Dr. HENRY indeed adduces in favour of the opinion of agents affecting the muscles only through the intervention of the nerves, a fact which is well ascertained, and which I amongst others have frequently witnessed, that the excitability of distant muscles may be impaired by chemical agents applied to the sentient extremities of the nerves; but here the brain and spinal marrow intervene between the nerves affected by the agent, and the muscles influenced. The effect of the agent is communicated through the nerves to these organs, and the debility of the muscles is the effect of the morbid impression made on them.

For the same reason a similar effect is produced on the muscles by what surgeons call concussion of the brain. A strong solution of opium or tobacco thrown into the cavity of the abdomen, or suddenly applied to any other extensive and highly sensible surface, has the same effect on the muscles as a blow on the head. It affects them in consequence of the brain and spinal marrow being influenced, and probably, where the cause is most powerful, having their finer mechanism deranged by the impression made on the sentient extremities of the nerves; so that we have here only an instance of a well-known fact, that certain affections of the brain and spinal marrow are capable of impairing the excitability of the muscles through the medium of the nerves, the only medium of course through which they can operate*.

How can we suppose, it has been said in support of the same opinion, that a muscle covered by a membrane of condensed cellular substance, or in other instances by the more complicated serous or mucous membrane, should be affected by a chemical agent applied to the opposite surface of such membranes, if not through the medium of its nerves?

If the agent does not pass through the membrane, and is thus immediately applied to the fibres of the muscle, by what other means can the effect be produced? We have just seen that the nerves are incapable of communicating the impression except where the brain and spinal marrow intervene. Besides, those who adopt this explanation forget that in many instances at least, I believe in all, the agent, independently of transudation through membranes, is as little in contact with the nerves as with the muscular fibres. The cuticle possesses neither nerves nor vessels, yet, through it, chemical agents influence the muscles; and are not such agents actually received through the cuticle and conveyed into the mass of blood?

It seems to be a general law of the animal system, that all membranes are pervaded by certain chemical agents. The air comes not in contact with the blood unless it be through the membranes of the lungs; yet who doubts that the changes effected in this organ are the consequence of transmission of an agent to or from the blood?

When a more stimulating blood produces a more powerful action of the heart and blood-vessels, can it arise from any other cause than the transmission

* Philosophical Transactions for 1815, and Experimental Inquiry.

of the more stimulating agent through the fine membrane which lines these cavities? Without such transmission it is no more in immediate contact with the nerves than with the muscular fibres; and if it were, we know that the impression it makes could not be conveyed through them. The membrane which lines the internal is more delicate than that which lines the external surface of the hollow muscles, and, as we might à priori expect, the agent pervades the former more readily than the latter. Hence it seems to be that a strong solution of opium thrown into the cavities of the heart, intestines, &c., immediately destroys their power; while applied to their external surface it makes no, or comparatively little, impression on it, the final cause of which it is not difficult to perceive. It is necessary that the muscular fibres of those cavities should be exposed to the stimulus of their contents, but their external surface only requires to be supported by the firmness of its membranes.

Must not the chemical agents which influence the nose, the mouth, the fauces, the bowels, the bladder, &c. pervade the fine membranes which line them before they can act either on their nerves or vessels?

There appear to be but two modes of impairing the excitability of the muscles independently of the immediate application of the agent to the muscular fibre itself; the one I have just had occasion to mention, the effect of powerful agents on the brain and spinal marrow, acting through them on the muscles; the other, the excessive excitement of their own function, too powerful and long-continued contractions.

The poisons which impair their excitability all affect them in one or both of these ways, except as far as they act by their immediate application to them. Opium, which was employed by Dr. HENRY in the experiments just referred to, acts in both ways. But the exhaustion produced in the muscles of voluntary motion when an animal is killed by opium, although in part arising directly from its effects on the brain and spinal marrow, particularly when it has been suddenly applied to a very sensible and extensive surface, is, under other circumstances, chiefly the consequence of the violent spasms excited in them.

The spasms produced by an over-dose of opium always assume the form of opisthotonos, but they are still more subject to remissions than in the disease properly so called; and, as in that disease, they are, during the remissions, often readily renewed by the slightest causes. Even the touch of the finger,

although the sensibility, as far as feeling is concerned, is wholly destroyed, is sufficient for this purpose ; so that in making the experiment the spasms may be rendered more or less frequent according to the circumstances in which the animal is placed. On examining the state of the muscles of voluntary motion after death, as I have repeatedly done, in animals killed by opium, one being left undisturbed, while in another a constant succession of spasms had been maintained, their excitability in the former I have found little, in the latter greatly impaired. All will admit that the general spasms here arise from the state induced on the brain and spinal marrow by the opium, and not from any particular change in the nerves ; any cause exciting by its action on the former organs, or in any other way, the same contractions, would, we know, produce the same exhaustion.

On reviewing all that has been said, it appears that we have no reason to suppose that the nervous influence excites the muscles on any other principle than that on which all other stimulants operate, which it also resembles in the circumstance of its excessive application acting as a sedative. I have been at much pains, in the last edition of my Inquiry into the Laws of the Vital Functions, to point out that all agents capable of influencing either the nervous or muscular system, and whether they make their first impression on the mind or body, act either as stimulant or sedative according to the degree in which they are applied, the stimulant effect always arising from the less, the sedative from the greater application of them, and different agents being better fitted to produce the one or other of these effects.

Thus with respect to the nervous influence, the more powerful, within certain limits, the action of the agent on the brain and spinal marrow, the greater is the stimulant effect on both classes of muscles ; but if it be extreme, as when a severe blow is inflicted on the head, instead of exciting, it impairs their power, in the same way that they are powerfully excited by a moderate application of electricity, but deprived of all power by its excessive application.

The foregoing observations are strikingly illustrated by the facts which have been ascertained respecting the nature of the nervous influence.

As this influence has been found to perform its functions in the animal economy after it has been made to pass through a space not less than a quarter of

an inch between the divided ends of a nerve, we must suppose either that, like magnetism or gravitation, it is capable of extending to a distance from the body in which it exists, or of passing through other conductors than the nerves. Were the former opinion correct, the influence of a nerve would extend in all directions, which we know not to be the fact, nor would the presence of the nerve be necessary to its functions, which we uniformly find to be the case. The conclusion then from the foregoing fact is unavoidable, that the nervous influence is capable of its functions after having passed through, and consequently existed in, other conductors than the nerves. It is therefore not peculiar to the nervous system, but capable of existing elsewhere, and consequently is not to be regarded as, strictly speaking, one of the vital powers of the animal body, but as an agent employed by them. On the other hand we find that voltaic electricity, applied under the same circumstances, is capable of all its functions, of exciting the muscles, of causing an evolution of caloric from arterial blood, of forming from the blood the various secreted fluids, and maintaining all the other processes of assimilation on which the healthy structure of every part depends*.

A vital power has no existence except in the particular mechanism to which it belongs, and its functions are of a nature which admits of the substitution of no other power. The muscular power has no existence but in the muscular fibre. The peculiar powers of the brain and spinal marrow are inseparable from these organs; and for none of these powers can any other power be substituted.

The nervous influence is therefore an inanimate agent, if this expression may be used; that is, one capable of existing in inanimate nature, and, consequently, independently of the mechanism to which in the animal economy it belongs, and the functions of which can be performed by an agent which we know to be of this description; and, as appears from what has just been said, the action of this influence on the muscles obeys the same laws as that of other inanimate agents capable of exciting them.

The results of the experiments, on which the foregoing positions rest, are stated in papers which the Society did me the honour to publish in the Philo-

* Philosophical Transactions for 1817, 1822, 1827, 1828 and 1829; and Experimental Inquiry.

sophical Transactions just referred to, and detailed more at length in the third edition of my Inquiry into the Laws of the Vital Functions. These experiments were publicly repeated with the same results at the Royal Institution in the presence of many of the first physiologists of this country, Sir HUMPHRY DAVY, Mr. ANDREW KNIGHT, Mr. BRODIE, &c.*, and also on a larger scale, a great variety of animals being employed, by M. BRESCHET and other physiologists at Paris†.

When the nature of the preceding positions,—for it will be admitted that the manner in which they have been ascertained precludes any doubt of their accuracy,—and the necessary inferences from them are carefully considered, it will be found difficult to deny that the nervous influence and voltaic electricity are powers of the same nature, although I have not been able to succeed in causing the former to affect the galvanoscope. It is found in like manner that the electricity of electric animals is incapable of affecting the electrometer‡.

As the nervous influence is capable of existing independently of the organs in which it operates in the living animal, and therefore is not a power peculiar to these organs, it must either be a power *sui generis* yet capable of existing in textures of the most dissimilar description, or one of those powers employed by nature in her other operations. All will admit that the latter supposition is the most probable. The better we understand the operations of nature the more simple we find them. We see an endless variety of results from modifications of the same principle. Whether the most probable opinion is here the correct one, can only be ascertained by determining whether any one of the powers which operate in the production of other phenomena is

* Journal of the Royal Institution, Nos. XXII. and XXIII. vol. xi. and xii.

† Recherches Expérimentales sur les fonctions du système nerveux. 1^{er} Mémoire; “De l’Influence du système nerveux sur la Digestion Stomachale,” par M. BRESCHET, D.M.P. Chef des Travaux Anatomiques de la Faculté de Médecine de Paris, etc. H. MILNE EDWARDS, D.M.P. et VAVASSEUR, D.M.P. (“Mémoire lu à la Société Philomatique le 2 Août, 1823,”) “Extrait des Archives Générales de Médecine, Août, 1823.”

‡ A paper entitled, “An account of some experiments and observations on the Torpedo (*Raia Torpedo*, LINN.) by JOHN DAVY, M.D. F.R.S. Assistant Inspector of Army Hospitals,” in the Philosophical Transactions for 1832. In page 262, Dr. DAVY observes, “In accordance with Mr. Walsh and my brother, I have in no instance seen the Torpedo affect the common electrometer, or exhibit any the slightest indications of a power of attraction and repulsion in air.”

capable of all the functions of the nervous influence. When it is found that such a power exists, all doubt must necessarily cease.

I have, as far as I know, in the preceding paper laid before the Society, or referred to, all the circumstances which ought to influence our judgement respecting the relation which subsists between the nervous and muscular systems of the more perfect animals, and the nature of the influence by which it is maintained; and shall conclude by shortly recapitulating the more important results.

It appears from all the facts which have been stated or referred to,—

That the power of the muscles both of voluntary and involuntary motion is independent of the nervous system; but that both are subjected to its influence, this influence being the constant stimulant in the functions of the former, but only an occasional stimulant in those of the latter, which in their ordinary functions are excited by stimulants peculiar to themselves.

That to the muscles of voluntary motion it is supplied from those parts of the brain and spinal marrow, from which the nerve of the particular muscle takes its rise; to each of the muscles of involuntary motion, from every part both of the brain and spinal marrow.

That these organs are the only active parts of the nervous system, and that the cerebral and spinal nerves on the one hand, and the ganglionic nerves with their ganglions and plexuses on the other, are only the channels through which their influence is conveyed, the power of both systems of nerves being at all times proportioned to the excitement of the brain and spinal marrow, and soon ceasing, and not to be renewed when their influence is withdrawn, and being uninfluenced by causes acting independently of these organs on either set of the nerves themselves or on the ganglions and plexuses.

That the ganglionic system of nerves, with their ganglions and plexuses, is the means of combining the influence of every part of the brain and spinal marrow, and bestowing it on the muscles of involuntary motion, as well as on the various secreting and other assimilating organs, those muscles being sub-

servient to the functions of these organs, which it appears, from direct experiment, require for their due performance the combined influence of every part of the brain and spinal marrow.

That the manner in which the nervous influence affects the muscular fibre is not essentially different from that in which it is affected by other stimulants and sedatives.

That this influence is not an agent peculiar to the nervous system, but capable of existing elsewhere, and consequently not a vital power properly so called ; which further appears from an agent which operates in inanimate nature being capable of all its functions.

That the brain and spinal marrow therefore, so far from bestowing on the muscular fibre its power, only supply an inanimate agent which, like all other such agents capable of affecting it, acts on it either as a stimulant or sedative, according to the degree in which it is applied ; and that the whole of the facts relating to this agent, prove its identity with voltaic electricity, which has been found experimentally to be capable of all its functions.

WE cannot review the phenomena of the animal economy without being struck with the extent and variety of the functions of the nervous influence. We not only find the intercourse between the animal and the external world maintained by it, the heart and vessels subjected to its controul, and secretion and the other assimilating processes immediately dependent on it*, but that by its means the animal body is formed into a whole, every part of it being capable of influencing every other. Can we be surprised then that it more extensively than any of the other powers of that body influences its morbid states? In two treatises on the various effects of Indigestion and on the prevention of Organic Diseases, I have endeavoured to point out the share it has both in

* So completely are the assimilating functions dependent on the nervous influence, that if the eighth pair of nerves be divided in the neck, and one of the divided ends folded back, in order to prevent any passage of nervous influence between them, the structure of the lungs will, in from fifteen to twenty hours, be so deranged, that in many places not even a vestige of their natural structure will remain. When, however, voltaic electricity is sent through the lungs as soon as their nerves are divided, and maintained in such power as supports a gentle twitching in the fore legs of the animal, the structure of the lungs is preserved, being found, in all respects, as perfect as if their nerves had remained uninjured.—Philosophical Transactions for 1822 and 1827, and Experimental Inquiry.

the production and in the extensive and varied effects of these derangements, and how much therefore their proper treatment depends on correct views of its agency.

From all that has been said, we are unavoidably led to the conclusion that the same principle which operates so extensively in other parts of nature, no less extensively operates in our own frames. It is capable of acting in concert with the vital powers, properly so called, as well as with the other powers of inanimate nature, thus forming the link, if I may be allowed the expression, by which these powers, so different in their nature, are enabled to co-operate.

In an Appendix to the seventh edition of my Treatise on Indigestion, I have at some length considered that function of the nervous influence by which the animal body is formed into a whole, and the manner in which it so extensively influences the phenomena and treatment of diseases.

Errata in the following paper.

Page 80, line 7, *for* it *read* the exhaustion.

—— line 8, *for* it, (last word but one in the line,) *read* sleep.